Hoang et al. Mekong River flow and hydrological extremes under climate change

Authors' responses to comments

By Long Phi Hoang

long.hoang@wur.nl

Responses to Editor's comments

We highly appreciate the editor's effort in handling the review process and providing constructive comments on the manuscript. We have addressed all comments and revised the manuscript accordingly, as described below.

Comment: The paper aims to fill the knowledge gaps in understanding hydrologic regime under climate change by using the newest IPCC assessment results .Generally the paper is well written with clear objectives. The authors use the routine methodology well developed in the literature, i.e., doing the hydrological modelling forced by ensembles of GCMs outputs. The results are clearly stated, with obvious difference from previous assessment with CMIP3. They have important implications for water resources management as well as disaster prevention.

Generally I agree with the Referees' comments, and the authors should adequately address all the comments including the following ones:

Response: We have addressed all comments from the reviewers in the revised manuscript, as described in our responses to reviewers' comment.

Comment#1: Similar with the Referee #1, one of my major concern is the uncertainty comparison between CMIP5 and CMIP3. The authors should highlight improvement made by the new study in a solid method.

Response: We have now done further analyses and added a new section (*5.1 Comparison: Impact signal and improvements in uncertainties*) to show important improvements in uncertainty in our study compared to earlier CMIP3-based studies. Uncertainty improvements were highlighted by comparing impact signal's range; cross-scenarios/GCMs agreement on directional changes; and coefficient of variation in the simulated discharges from the available scenario ensembles. All in all, our analyses show reduced uncertainties and more robust impact signals from this CMIP5-based assessment compared to earlier CMIP3-based studies.

Added text:

"*Furthermore, the projected impact signals in this study exhibit less uncertainty compared to similar CMIP3-based assessments. A cross-study comparison (see Table 4) for the representative Kratie station shows that both the impact signal's range and cross-scenarios agreement on directional changes improved markedly in this CMIP5-based study. In particular, the ranges of annual discharge change, i.e. 3% to 8% (RCP4.5) and -7% to 11% (RCP8.5) are typically smaller than those projected by earlier studies including Eastham et al. (2008), Kingston et al. (2011), Lauri et al., (2012) and Thompson et al., (2013). Similarly, the projected precipitation changes also show less uncertainty in the CMIP5 scenarios compared to the CMIP3 scenarios. Additionally, directional discharge changes also shows better consensus in this study. The CMIP5-based ensemble's impact signal (i.e. increasing annual discharge) is supported by nine out of ten individual scenarios, whereas other studies show relatively lower consensus. Lastly, we compared uncertainty in hydrological extremes by calculating the coefficient of variation for projected yearly peak discharges between studies. Due to limited data availability, we only compared our study with Lauri et al. (2012). Both studies have ensembles of ten projections, grouped into a mid-range scenario (i.e. RCP4.5 versus SRES-B1) and a high scenario (i.e. RCP8.5 versus SRES-A1B). Overall, our CMIP5-based projection exhibits lower uncertainty, shown by lower coefficients of variation for both the mid-range scenarios (24% versus 38%) and the high scenario (25% versus 38%). Reduced uncertainty detected in our study is also in line with studies by Sperber et al., (2012) and Shabeh Uh et al. (2015) where they found improved representations of the Asian summer monsoon by the CMIP5 models.*"

Table 4. Comparing projected precipitation and discharge changes across studies.

	Eastham et al. 2008	*Kingston et al. 2011*	*Lauri et al. 2012*	*Thompson et al. 2013*	***Hoang et al. 2015 (this study)***
Range of annual precipitation change	*0.5% to 36% (A1B)*	*-3% to 10% (up to 6°C warming)*	*-2.5% to 8.6% (A1B)* *1.2% to 5.8% (B1)*	*-3% to 12.2% (2°C warming)*	***3% to 4% (RCP4.5)*** ***-3% to 5% (RCP8.5)***
Scenarios projecting higher annual precipitation	*Not available*	*4 out of 7*	*9 out of 10*	*4 out of 7*	***9 out of 10***
Range of annual discharge change	*Not available*	*-5.4% to 4.5% (up to 6°C warming)*	*-10.6% to 13.4% (A1B)* *-6.9% to 8.1 % (B1)*	*-14.7% to +8.2% (2°C warming)*	***3% to 8% (RCP4.5)*** ***-7% to 11% (RCP8.5)***
Scenarios projecting higher annual discharge	*Majority of GCMs show increasing trend*	*4 out of 7*	*7 out of 10*	*3 out of 7*	***9 out of 10***

Comment#2-1: Hydrological model is important for the study. A review of relevant hydrologic researches in Mekong area is preferred, especially in terms of model skill.

Response: We have added the following sentences to the Introduction session about existing hydrologic researches for the Mekong basin, focusing on reported model skills and simulation performance.

Added text: "*Compared to uncertainty in the future climate, uncertainty relating to hydrological models' schematization and parameterization seems less important for the Mekong basin. Regarding hydrological model's skill, many studies including Hoanh et al., 2010; Västilä et al., 2010; Kingston et al. (2011) and Lauri et al. (2012) reported sufficient performance in capturing the dynamics of the Mekong's hydrology. Several studies also reported lower modelling skill in more upstream stations (e.g. Chiang Saen) compared to more downstream stations including Kingston et al. (2011) and Lauri et al. (2012).*"

Comment#2-2: A related comment is the uncertainty of hydrologic modelling. As we can see from the FDC in Figure 2, the high flows and low flows are not sufficiently replicated. How can you say Q5 and Q95 indices are all within acceptable ranges. It requires more supports from literatures. Also, how this uncertainty influences the evaluation results needs more discussions.

Response: We have revised the text to better acknowledge the relatively lower model performance at Chiang Saen station, which is also seen in other modelling studies including Kingston et al., 2011; Lauri et al., 2012. We also added further explanation and discussion to this point in both the Results and Discussion sections.

Revised/added text:

(Section 4.1): "*Similarly, the relative biases in total discharge, and the high flows (Q5) and low flows (Q95) indices are all within acceptable ranges, except for relatively lower performance at the most upstream Chiang Saen station*."

(Section 5.3): "*Given relatively lower modelling skill at Chiang Saen, interpreting hydrological impact signal at this station requires extra caution.*"

Comment#3: L21-23 Pg20**:** dam failure is an extreme case for dam operation. This 'extreme' is not the same 'extreme' in the paper title. The Q5 extreme is usually related to the flood regulation with the aim to protect the downstream area, while the extremes leading to dam failure often mean Q0.01 (1000-year return period) or even bigger events (e.g., 10000-year return period). The paper does not talk about these extreme extremes, so the discussion and conclusion about the dam failure is not in the proper context.

Response: We fully agree with the editor comment. We have removed the text on dam failure from the manuscript.

Comment#4: Ln19-20 Pg6: South-West or South-East?

Response: We have corrected this to "*South-West*". Thank you very much for checking the manuscript very carefully!

Responses to Reviewer#1 comments

We highly appreciate Reviewer#1 for his/her dedicated reviews and constructive comments on the manuscript. We have addressed all comments and revised the manuscript accordingly, as described below.

Comment: The manuscript in discussion presents an assessment of the impacts of CMIP 5 climate change scenarios on extreme events of river flows through the Mekong River Basin. The study uses an ensemble of 10 scenarios, which were properly downscaled, biased corrected, and used to run a well calibrated and validated hydrological model. Overall, I think that this manuscript represents an important contribution to the understanding of hydrological impacts of climate change in the Mekong, providing also a robust and replicable methodology to be used in river basins elsewhere. As the authors clearly stated, this is one of the first hydrological studies in the Mekong using CMIP5 scenarios and thus of critical value to the hydrology community of this region. Among previous studies of climate change in the Mekong and similar river basins, in fact, this study sets a new bar of standards for comparative studies to follow. In general, the paper follows a clear outline, its justification is clear, methods well explained, and results sufficiently robust. There are, however, two major points that I suggest the authors clarify and expand on in the manuscript.

First, one of the primary justifications for this study –and the use of CMIP5 scenarios–is the large uncertainty associated with previous projections. The hope is that this new set of scenarios could show if CMIP5 models and scenarios have a better agreement among them and thus decrease uncertainty in climate predictions. This aspect, however, remained largely unresolved from the manuscript. I recommend that the authors assess and discuss in more detail if the new set of scenarios are actually alleviating the uncertainty issue in comparison to the previous assessments with CMIP3 scenarios, which is a finding that clearly could bring serious implications and challenges to water managers on the ground.

Response: We fully agree with Reviewer#1 that the CMIP5 versus CMIP3 uncertainties deserve better attention in the manuscript. We therefore further analysed our climate and hydrological impact signals and compare these to similar CMIP3-based assessments. We compared our results with four CMIP3-based assessments for the Mekong region, including Eastham et al. (2008), Kingston et al. (2011), Lauri et al. (2012) and Thompson et al. (2013). These studies include multiple GCMs and provide results that can be reasonably compared to our results. We have added a separate section (*5.1 Comparison: Impact signal and improvements in uncertainties*) to illustrate and discuss improvements in uncertainties relating to climate and hydrological impact projection. We also added *Table 4* to the manuscript present the cross-studies comparison.

Table 4. Comparing projected precipitation and discharge changes across studies.

	Eastham et al. 2008	*Kingston et al. 2011*	*Lauri et al. 2012*	*Thompson et al. 2013*	***Hoang et al. 2015 (this study)***
Range of annual precipitation change	*0.5% to 36% (A1B)*	*-3% to 10% (up to 6°C warming)*	*-2.5% to 8.6% (A1B)* *1.2% to 5.8% (B1)*	*-3% to 12.2% (2°C warming)*	***3% to 4% (RCP4.5)*** ***-3% to 5% (RCP8.5)***
Scenarios projecting higher annual precipitation	*Not available*	*4 out of 7*	*9 out of 10*	*4 out of 7*	***9 out of 10***
Range of annual discharge change	*Not available*	*-5.4% to 4.5% (up to 6°C warming)*	*-10.6% to 13.4% (A1B)* *-6.9% to 8.1 % (B1)*	*-14.7% to +8.2% (2°C warming)*	***3% to 8% (RCP4.5)*** ***-7% to 11% (RCP8.5)***
Scenarios projecting higher annual discharge	*Majority of GCMs show increasing trend*	*4 out of 7*	*7 out of 10*	*3 out of 7*	***9 out of 10***

Our CMIP5-CMIP3 comparison shows that both the projection range and cross-GCMs/scenarios agreement on impact signal improve markedly in our CMIP5 assessment. In particular, our projected range for basin wide annual precipitation change is typically smaller than other CMIP3 assessments, implying better consensus in CMIP5 compared to CMIP3. Similarly, cross-GCMs/scenarios agreement on yearly discharge changes at Kratie also show best consensus in our assessment. . Additionally, we compared uncertainty in hydrological extremes by calculating the coefficient of variation for projected yearly peak discharges with results from Lauri et al. (2012). Data from other studies for such comparison is not available. Overall, our CMIP5-based projection exhibits lower uncertainty, shown by lower coefficients of variation for all considered scenario ensembles. Reduced uncertainties detected in our study are also in line with the expectations from Sperber et al., (2012) and Shabeh Uh et al. (2015) where the authors found a better representation of the summer monsoon under the CMIP5 models for the Mekong basin region. All in all, cross-studies comparison suggest reduced uncertainties and more robust impact signals out of our CMIP5-based GCMs/scenarios ensemble compared to earlier CMIP3-based studies. We have dedicated substantial text to highlight this matter in the manuscript, mostly in the results and discussion sections.

Comment: That brings me to the second major issue; while the authors briefly discussed some of the implications for water management, I thought that this discussion was a bit too general and short, given how crucial these findings are for the region. With the exception of a few comments, benefits and impacts to the water-depended sectors that the authors mentioned in the introduction –fisheries, agriculture, and hydropower– were largely ignored from the discussion.

Response: We have substantially revised the discussion session to better address the implications for water management. In particular, we added one separate section (*5.2 Implications for water management*) to discuss impacts of discharge changes on flood risk, agricultural production, hydropower development and safety, and aquatic ecosystems in the lower Mekong region. We have also added the potentially important impacts of flow reduction in early wet season on the sediment and nutrient dynamics, following Reviewer#1's suggestion.

In addition to these general comments, there are a number of more punctual issues that I would like to bring to the attention of the authors:

Comment -Abstract: in the case that the manuscript is updated based on the two major comments above, be sure that those are incorporated in the abstract

Response: We have included additional text to the abstract and conclusion to better inform readers about (1) improvements in CMIP5-based projection's uncertainties and (2) implications of our findings for safety risks, water resources management and aquatic ecosystems.

Added text:

"*The scenarios ensemble, however, shows reduced uncertainties in climate projection and hydrological impacts compared to earlier CMIP3-based assessments.*"

"*Climate change induced hydrological changes will have important implications for safety, economic development and ecosystem dynamics and thus require special attention in climate change adaptation and water management.*"

Comment -11655 l. 26: Higher precipitation amount per unit area

Response: Corrected to: "*This implies that the basin receives higher precipitation amount per unit area, owing to its dominant tropical monsoon climate (Adamson et al., 2009; Renaud et al., 2012)*"

Comment -11656 l. 15: None of the 3 references provided here actually documented ecosystem and/or agricultural productivity in the lower Mekong. Please correct

Response: Thank you for checking the manuscript very carefully! We have corrected and added more relevant references to the manuscript.

Revised text: "*Seasonal variation in river flow, especially the flood pulse occurring in the downstream delta (i.e. the Tonle Sap Lake in Cambodia and the Vietnamese Mekong delta), supports a highly productive aquatic ecosystem and one of the world's major rice production area (Lamberts and Koponen, 2008; Arias et al., 2012)*"

Comment -11656 l. 19: Similar to the above, these two references do not relate to economic productivity of the Mekong. In fact, the Lamberts and Koponen (2008) reference will serve well in line 15 mentioned above

Response: We have added more relevant references to the manuscript.

Comment -11657 l. 12: What are the soil surface processes and associated methods used in VMOD? Such information was provided with regards to PET, so it is probably good to include here for consistency

Response: Following Reviewer#1's suggestion, we have added more details on soil surface processes and associated methods to the method section.

Added text: "*The soil is simulated as two distinctive layers and soil surface processes are simulated following Dingman (1994). After calculating the water balance, runoff is routed from cell to cell and finally into the river network.*"

Comment -11657 l. 21: A relatively outdated land cover map was used to run the model. Are there any justifications for the use of this map? There have been major land transformations in the Mekong in the past decade and I wonder if the authors carried out any sensitivity test –as they did for precipitation– with regards to this input.

Response: We selected the GL2000 land cover map in consideration of the calibration and validation period (1981-2001). The GL2000 provides land cover data that is most suitable to our time period of interest. Furthermore, we agree with Reviewer#1 that recent changes in the land use system, especially at the Lower Mekong countries, may have considerable impacts on river flows. We added our acknowledgements to this matter in the discussion and include this in our on-going complementary study, where future land use change, particularly agricultural land expansion is included. Regarding historic land use change, we did not account for this factor in our hydrological simulations due to unavailable data. Given the relatively robust performance of the model during calibration and validation, we believe that the modelling set up is reliable for our research purpose.

Comment -11658: what is the justification for the calibration/validation period? The simulations were carried out from the early 1970s, time from which there are flow records that could have been used

Response: We restrained from going too far to the past (i.e. 1970s) in our calibration and validation exercises to make sure that measured data is available for all seven mainstream stations (Chiang Saen, Vientiane, Nakhon Phanom, Mukdahan, Pakse, StungTreng and Kratie). Additionally, land cover situation in the 1970s might be too different from our land use data around 2000. Given relatively good performance of the hydrological model over a 20-yr period for calibration and validation (as shown in Table 2), we believe that the model setup and parameterizations is sufficiently reliable.

Comment -11658: Was the calibration done manually only? No systematic/automatic routines?

Response: We only calibrated VMod manually (information added to manuscript). The autocalibration feature of VMod requires many runs and thus long computing time. Since the model set up and parameterizations for the Mekong basin have been proved very robust in an earlier study, i.e. Lauri et al., (2012), we believe that the model's performance is sufficiently reliable for our assessment.

Comment -11661: Despite the model performing very well in the lower stations, the discharge biased at Chiang Sean concerns me. There are a number of publications in recent years that have shown a significant increase in dry season flows at this exact location as a result of the construction of dams in the upper Mekong in China. Such effect could directly explain the discrepancies shown in the flood duration curves comparison in fig. 2. Based on the published evidence this seems to be a factor that the authors should considered or at least discussed about.

Response: We thank Reviewer#1 for this excellent comment and suggestion on possible linkages between the upstream Chinese dams and the biases in simulated discharge at Chiang Saen station. Recent studies including Adamson (2001), Lauri et al. (2012) and Räsänen et al. (2012) found substantial increases in dry season flow at this location due to hydropower dams operation. These increases are in line with the biases shown in our simulated river discharge where measured flow tends to be higher than simulated flow during dry season (shown in Figure 2). We have added our explanations and discussions regarding this matter to the manuscript and referred to recent supporting studies.

Added text: "*Low flow biases at Chiang Saen could be explained by unaccounted flow regulation by upstream hydropower dams during the dry season, as suggested by Adamson (2001), Lauri et al. (2012) and Räsänen et al. (2012).*"

Comment -11663 l. 5: Spatial variability in rainfall is mentioned here. That is a critical point that I suggest is discussed in more detail in the discussion. In particular, a reduction in rainfall in the lower Mekong could have serious implications for rainfed agriculture, which does occur in large areas.

Response: We agree with Reviewer#1 that rainfall reduction in parts of the lower Mekong can have substantial implications. We have added more text to further discuss the implications of rainfall reduction in some parts of the lower Mekong.

Added text: "*Lastly, rainfall reduction in some areas of the lower Mekong could damage agricultural production, especially rainfed agriculture.*"

Comment -11664 l.8 : Following up with implications to agriculture, the authors mentioned that one of the only projections for which all scenarios agreed was a reduction in flow in June. Flows during the early wet season are critical for both ecological and agricultural productivity, bringing the first flush of water and sediments critical for growth.

Response: Following Reviewer#1's suggestion, we have added more text on the implication of flow reduction in June for agricultural activities.

Added text: "*Additionally, projected discharge reduction at the beginning of the wet season (i.e. in June) might have negative impacts on ecological and agricultural productivity. Flow alteration in the early wet season will likely change the sediment and nutrient dynamics in the downstream floodplains, which are very important for existing ecosystems and agricultural practices (Arias et al., 2012).*"

Comment -Fig. 1: For the readers that are not familiar with the Mekong, it would probably be helpful to show country boundaries

Response: We have added the country boundary to Figure 1.

Comment -Fig. 5: When printed in regular A4 paper, this figure is very difficult to read. I suggest using a much larger format. In addition, I felt that the ensemble mean lines was not showing a clear message; I recommend removing the mean lines from the discharge graphs in the first two columns, and in the third columns (% change), show also the error bars associated with the monthly difference among GCMs

Response: We thank Reviewer#1 for the useful and practical suggestions to improve Figure 5. We have enlarged this figure to improve readability. By enlarging the figure panels, the mean lines as well as the projection range are now more visible. We believe that the more visible projection range (i.e.

shaded area) essentially illustrate the same information as the error bars, and thus we restrain from adding error bars to the figure. Due to limited space, we now moved the percentage change panel to the supplementary S2.

Revised Figure 5:

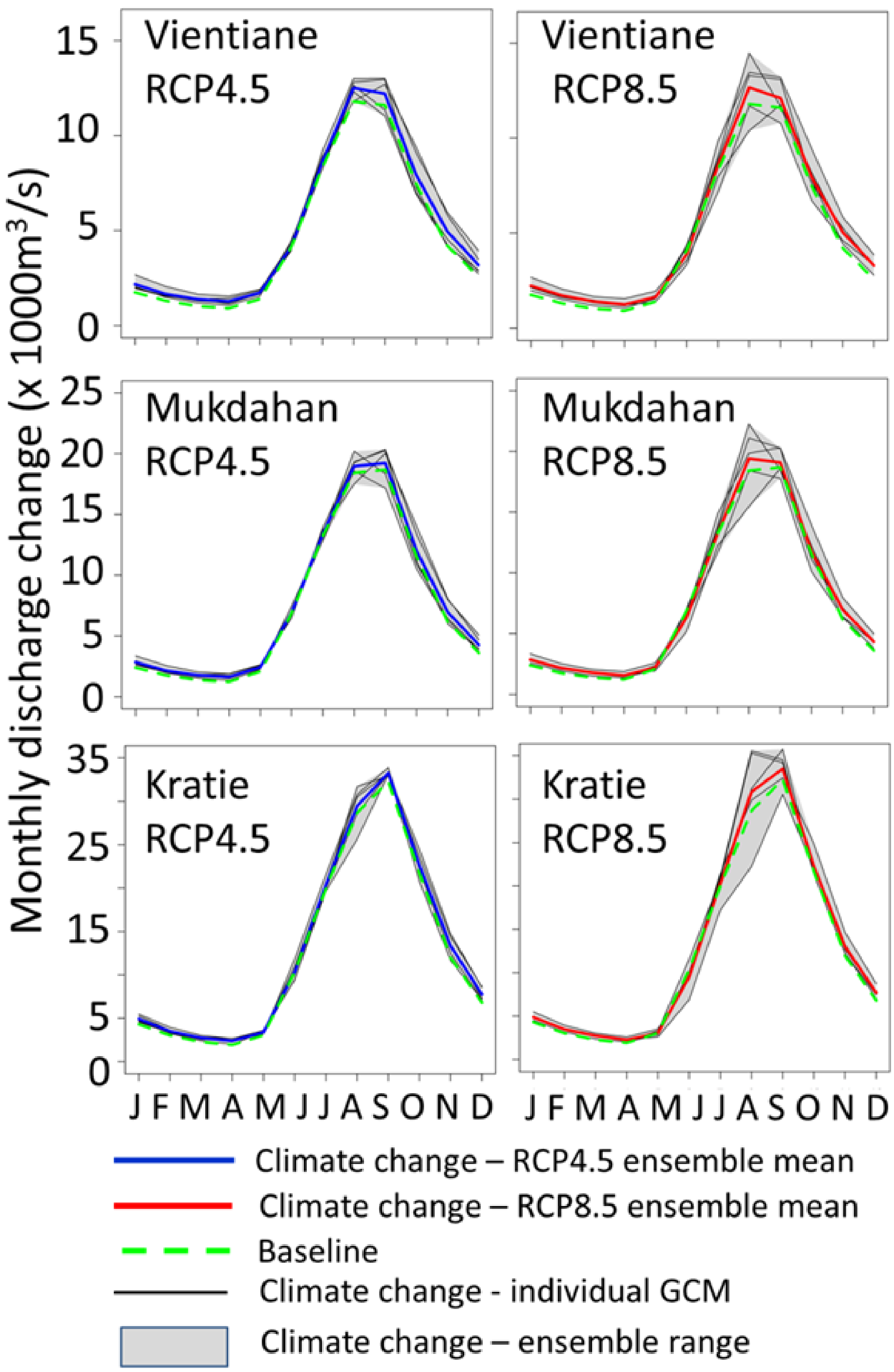

Figure 5. Projected monthly river discharge (left and middle panels) and relative changes (right panel) under climate change for 2036-2065 relative to 1971-2000.

Responses to reviewer#2 comments

We highly appreciate Reviewer#2 for his/her dedicated reviews and constructive comments on the manuscript. We have revised the manuscript following the comments and suggestions, as described below.

This article discussed the climate change impacts on river flow and hydrological extremes in Mekong river basin. The topic is important and has been studied by many researchers. Compared to previous studies, this work attempted to reduce the uncertainty involved in climate projection using the CMIP5 data, and also highlighted the influences on extreme events. In general, its organization is straightforward, the methodology looks reasonable, and results were clearly explained.

A few limitations:

Comment#1. This study was motivated to reduce the uncertainty involved in previous studies. However, was this goal achieved in this paper? I'd say very limited. The authors used the most recent climate projection data, ran them with established models, and then performed analysis. The only advance s compared to previous studies is the climate data, which results the paper less innovative.

Response: We acknowledge Reviewer#2's point to focus more on uncertainties in climate and hydrological impact projections in the manuscript. We have done further analyses to illustrate that our CMIP5-based assessment exhibit lower uncertainties compared to similar CMIP3-based assessments, both in climate change and hydrological impact signals. We have added a separate section (*5.1 Comparison: Impact signal and improvements in uncertainties*) to illustrate and discuss improvements in uncertainties relating to climate and hydrological impact projection. We also added Table 4 to the manuscript present the cross-studies comparison.

Our CMIP5-CMIP3 comparison shows that both the projection range and cross-GCMs/scenarios agreement on impact signal improve markedly in our CMIP5 assessment. In particular, our projected range for basin wide annual precipitation change is typically smaller than other CMIP3 assessments, implying better consensus in CMIP5 compared to CMIP3. Similarly, cross-GCMs/scenarios agreement on yearly discharge changes at Kratie also show best consensus in our assessment. . Additionally, we compared uncertainty in hydrological extremes by calculating the coefficient of variation for projected yearly peak discharges with results from Lauri et al. (2012). Data from other studies for such comparison is not available. Overall, our CMIP5-based projection exhibits lower uncertainty, shown by lower coefficients of variation for all considered scenario ensembles. Reduced uncertainties detected in our study are also in line with the expectations from Sperber et al., (2012) and Shabeh Uh et al. (2015) where the authors found a better representation of the summer monsoon under the CMIP5 models for the Mekong basin region. All in all, cross-studies comparison suggest reduced uncertainties and more robust impact signals out of our CMIP5-based GCMs/scenarios ensemble compared to earlier CMIP3-based studies. We have dedicated substantial text to highlight this matter in the manuscript, mostly in the results and discussion sections.

Table 4. Comparing projected precipitation and discharge changes across studies.

	Eastham et al. 2008	*Kingston et al. 2011*	*Lauri et al. 2012*	*Thompson et al. 2013*	***Hoang et al. 2015 (this study)***
Range of annual precipitation change	*0.5% to 36% (A1B)*	*-3% to 10% (up to 6°C warming)*	*-2.5% to 8.6% (A1B)* *1.2% to 5.8% (B1)*	*-3% to 12.2% (2°C warming)*	***3% to 4% (RCP4.5)*** ***-3% to 5% (RCP8.5)***
Scenarios projecting higher annual precipitation	*Not available*	*4 out of 7*	*9 out of 10*	*4 out of 7*	***9 out of 10***
Range of annual discharge change	*Not available*	*-5.4% to 4.5% (up to 6°C warming)*	*-10.6% to 13.4% (A1B)* *-6.9% to 8.1 % (B1)*	*-14.7% to +8.2% (2°C warming)*	***3% to 8% (RCP4.5)*** ***-7% to 11% (RCP8.5)***
Scenarios projecting higher annual discharge	*Majority of GCMs show increasing trend*	*4 out of 7*	*7 out of 10*	*3 out of 7*	***9 out of 10***

Next to using updated climate data, we have revised the abstract, discussion and conclusion sections to highlight another important innovative aspect regarding its focus on changes in hydrological extremes (both low and high flow conditions). We found robust evidences of increases in hydrological extremes, and therefore recommend a shift in research focus and water management, towards better attention to low-probability but high-damage events. We have also included these to the revised abstract:

Added text:

"*The scenarios ensemble, however, shows reduced uncertainties in climate projection and hydrological impacts compared to earlier CMIP3-based assessments.*"

"*Climate change induced hydrological changes will have important implications for safety, economic development and ecosystem dynamics and thus require special attention in climate change adaptation and water management.*"

Comment#2. The authors also mentioned the missing human part in the discussion. Anthropogenic factors such as land use change and hydropower operation affect the results significantly. It would greatly improve the value of the study if some of the effects can be integrated with the model.

Response: We fully agree with Reviewer#2's point on the potential impacts of anthropogenic factors on the Mekong's hydrology. Acknowledging these, we plan to implement a follow-up study, where land use change, particularly agricultural land expansion, and hydropower dams development in the Mekong basin are exclusively included. Regarding the current study, we focus solely on climate change in order to highlight the importance of this particular factor, as well as to establish the required physical boundary condition for further considering other anthropogenic factors. Furthermore, we have added more text to the manuscript to further acknowledge and discuss the possible impacts of anthropogenic factors on the Mekong's hydrology including land use change.

Specific comments:

Comment Page 11654, line 13: suggest revising the sentence

Response: Following Reviewer#2's suggestion, we have revised the sentence to improve readability: "*Notably, all earlier studies used the SRES emission scenarios (Nakicenovic et al., 2000), which were developed for the Climate Models Inter-comparison Project phase 3(CMIP3).*"

Comment Page 11658, line 3: do you consider land use change for the two calibration periods and how?

Response: We agree with Reviewer#2 that land use change in the past might have impacts on the river flow and including this factor could potentially improve calibration and validation results. However, we could not include land use change when calibrating and validating the hydrological model due to several technical reasons. Our calibration and validation cover the 1981-2001 period, when land use and land use change data is still rather limited for the Mekong region. Given unavailable temporally-continuous data, introducing different land use layers for different points in time will likely result in abrupt shifts in the simulated river flow, which is undesirable in our climate change impact assessment study. Given relatively good performance of the hydrological model during calibration and validation, we believe that the modelling setup is sufficiently reliable for our research objective.

Comment Page 11659, line 21: It is biased to assume that GCMs perform well in producing historical data would also do great in projection. If that is true, you don't need to select 5 GCMs.

Response: We agree with Reviewer#2 that well-performing GCMs for the historic period do not always imply good projection capacity. However, we believe that GCMs selection based on historic performance, which is a common choice amongst recent studies (e.g. Västilä et al., 2010; Lauri et al., 2012), is a relatively efficient and reliable approach. In particular, good performance for historic climate is an indication of better parameterizations, thus better capacity to capture the climatic features (e.g. monsoon driven precipitation) in the Mekong basin.

We have added extra information to the method section to explain this: "*We selected those GCMs that better reproduce historic tropical temperature and precipitation conditions, implying their suitability to be used in the Mekong region.*"

Comment Line 11660, line 9: what do you mean by "high climate change scenario"?

Response: Thank you for checking the manuscript very carefully! We have revised the sentence to "*The RCP8.5 is a high radiative forcing scenario assuming a rising radiative forcing leading to 8.5W/m2 by 2100 (Riahi et al., 2011).*"

Comment Suggest revision Figures: I'd suggest adding a spatial map showing discharge changes like figure 4 so as to better illustrate the results

Response: We thank Reviewer#2 for the suggestion. However, we think it is best to restrain from producing such maps for several reasons. We believe that spatial differences across the basin have been adequately reported throughout the manuscript, mostly by Table 3 (discharge changes at seven representative locations); Figures 5; 6; 7 and 8 (discharge changes at three locations representing upper, middle and lower sub-basins). Furthermore, presenting spatial data would require averaging data so as to avoid having too many figure panels. This averaging does not really match with our objective to present results on hydrological extremes, which are better presented at representative locations.

Comment: Land use is an important factor in hydrological modelling and expects to change with time. Do you include this in your model?

Response: Our hydrological model does account for land use situation in the modelled river basin, as mentioned in the methodology section. Regarding land use change, we agree with Reviewer#2 that this factor can have implications for hydrological change in the Mekong basin. However, in this paper we focus solely on climate change in order to highlight the importance of this particular factor, as well as to establish the required physical boundary condition for further considering other anthropogenic factors. Please also refer to our responses to your comment#2 above on this matter.

Responses to short comments from students group

We appreciate the students' interest to take our manuscript for their peer-review exercise and to provide their short comments. We have revised the manuscript following the relevant comments and suggestions, as described below.

HESS manuscript evaluation criteria

Comment#1. Does the paper address relevant scientific questions within the scope of HESS? Yes, the study of flow regimes in the Mekong basin and their response under climate change is within the scope of HESS. The scope of this journal is comprised of three major aspects, and this study falls exactly under the third category: the study of the interactions with human activity of all the processes, budgets, fluxes, and pathways as outlined above, and the options for influencing them in a sustainable manner, particularly in relation to floods, droughts, desertification, land degradation, eutrophication, and other aspects of global change.

Response: Response not needed.

Comment#2. Does the paper present novel concepts, ideas, tools or data? This paper does not present new concepts or ideas, but it does present new data. This paper repeats work that has been done before, that is, executing a hydrological impact assessment based on predictive climate change data (Västilä et al., 2010) and (Lauri et al., 2006; 2012), but the authors use the most recent CMIP5 Climate change scenarios to complete this assessment. In their use of the latest data to complete their study, they update the current understanding of the Mekong basin's behavior under climate change. While this new data could be valuable to those looking to manage the water resources in the Mekong Basin, this study is really more of a work of engineering, because new data are analysed by existing methods. However, this article is novel in its focus on hydrological extremes, since most previous studies focused only on changes at a monthly or seasonal timescale.

Response: We agree with the students that one key novelty of our research is its focus on hydrological extremes under climate change. Furthermore, we have revised the manuscript to highlight that the study, although taking a similar approach as other impact assessment studies, does convey new ideas and messages concerning understanding and managing hydrological extremes in the Mekong basin. As mentioned throughout the abstract, results, discussion and conclusion sessions, the study focused strongly on hydrological extremes and reported robust evidences of substantial increases in both high flow and low flow conditions. Given these, we also recommend a shift towards a stronger focus on quantifying and managing hydrological extremes in the discussion and conclusion sessions. To our knowledge, such shift in focuses has not explicitly addressed in earlier literatures due to uncertain impact signals from earlier CMIP3-based assessment.

Comment#3. Are substantial conclusions reached? There are two major conclusions reached in the article: The first is that temperature, precipitation, and discharge will all increase under climate change, but the variation between models highlights the need to be prepared for a variety of different scenarios. The second is that it is necessary to use an ensemble approach in hydrological assessments, to correct for the considerable differences in outcomes from the use of different GCMs. Neither of these conclusions is particularly groundbreaking, but it is certainly valuable to verify the behavior of the Mekong basin under climate change using the updated data.

Response: We think that the students seemed to miss one additional major conclusion of this study regarding substantial changes in hydrological extremes under climate change. We have added more text to further highlight this important conclusion in the manuscript.

Comment#4. Are the scientific methods and assumptions valid and clearly outlined? The outline is made very clear in the introduction, and the methodology used in setting up the model was very clearly explained and justified with citations of other similar work, particularly (Lauri et al., 2006; 2012). The use of different climate models was explained and the choice of models was clearly justified in the discussion section.

Response: Response not needed

Comment#5. Are the results sufficient to support the interpretations and conclusions? Yes, but it is worth noting that the conclusions are very broad, discussing general trends in the Mekong watershed and their general implications.

Response: We have further elaborate and sharpen the conclusion section, taking this comment into account.

Comment#6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? The article explains the methodology in depth, although it relies heavily on citing other studies (Lauri et al. 2006 and 2012) for more precise details of how exactly the model was set up.

Response: Since VMod is a standard, state-of-the-art distributed hydrological model's with detailed technical description in Lauri et al. (2006, 2012), the manuscript focused more on describing its applications to the Mekong Basin case, including substantial information on model calibration and validation.

Comment#7. Do the authors give proper credit to related work and clearly indicate their own /original contribution? Yes. They clearly explained and credited the previous work on which their model was built, and noted what aspects of their work and results were new or different from previous studies.

Response: Response not needed.

Comment#8. Does the title clearly reflect the contents of the papers? Very clearly.

Response: Response not needed.

Comment#9. Does the abstract provide a concise and complete summary? Yes. In general, the abstract was excellent, but it was not clear why the authors chose to note the annual change of +5% and +16%. This is the only numerical data presented in the abstract, but it does not appear to be the most important data in the article, and it doesn't actually add any substance to the abstract. For these reasons it may be preferable to remove this from the abstract.

Response: We thank the students for their comment on the inclusion of the hydrological impact signal range in the abstract. We have added our rationales behind this range in the abstract to illustrate a robust increasing trend in the Mekong's hydrology under climate change. This is an important finding and message that we would like to convey in the abstract, especially when earlier studies typically reported uncertain and contrasting impact signal under the older CMIP3 climate change projection.

Comment#10. Is the overall presentation well structured and clear? Overall, yes, the presentation is clear. One suggestion would be to move the section 2.1, which explains the characteristics of the watershed, into a new section titled "Study Area."

Response: We have moved this section out of Section 2 (Method) and put it in a separate section.

Comment#11. Is the language fluent and precise? Yes, very well written, except on page 11658 line 12, there is a "u" missing in "rain gauge".

Response: Corrected.

Comment#12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes, all formulae, symbols, abbreviations and units are correctly defined and used.

Response: No response needed.

Comment#13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes, the introduction could be reduced because it is repetitive in its discussion of water resource scarcity and impacts of climate change on socio-economic development. For example, on page 11653, lines 6-7, the authors say "Both demographic and economic trends imply an increasing importance of water resources for future socio-economic developments. (Pech and Sunada, 2008; Hoanh et al., 2010; Keskinen et al., 2010)." In the next paragraph, lines 11-13, they repeat this sentiment "Socio-economic developments in the Mekong River basin, however, are facing critical challenges relating to water resources, including hydrological changes caused by climate change (Keskinen et al., 2010; MRC, 2010; Västilä et al., 2010)."

Response: We have shortened the introduction session by (1) making the text more concise and (2) removing redundant information from the introduction. Regarding the similar sentences pointed out in the short comment, we have now removed the former sentence.

Comment: Figure 2 could be clarified by adding a time scale to the x-axis.

Response: We have added a timescale to Figure 2

Comment: Table 3 should instead be presented more like Figure 6. In Figure 6 we are able to see what each of the models predict, unlike in Table 3 where we can only see the ensemble mean, and then the minimum and maximum change for each station.

Response: We thank the students their suggestion to modify Table 3, however, we would like to restrain from modifying this table. We use Table 3 to give an overview of projected discharge change and the projection's range. We thus focus on presenting the ensemble means, and the ranges instead of describing the differences amongst individual scenarios and GCMs. We think that these differences are sufficiently described in Figure 6, Figure 7, Figure 8 and associated text.

Comment: Figure 5 is too small, it is very difficult to see and compare the different curves shown in the graph. In addition, showing the relative discharge change as a percentage is misleading because it shows that there are enormous changes taking place between January and April, when in reality, there are just small fluctuations in low flows. It would be better to display this information as absolute change, not as a percentage (or eventually both absolute and relative), because the reader is really interested in knowing where the large amplitude changes are taking place.

Response: We have enlarged this figure to improve readability. By enlarging the figure panels, the lines as well as the projection range are now more visible. Due to limited space, we now moved the percentage change panel to the supplementary note. Regarding data presentation as absolute and percentage change, we have sufficiently report the result in absolute values throughout the text. Additionally, we think that reporting relative discharge change is actually highly relevant, especially in the dry season. A reduction in low flows, which can have serious implications for ecosystems and agricultural activities, could be masked out when reporting in absolute terms since it is too small compared to the absolute changes in the wet season.

Comment: Abstract: In line 5, the authors say that this is "one of the first" hydrological impact assessments. If they say this, they should reference the other studies that completed assessments with CMIP5. However, because they don't have space (and it would be inappropriate) to reference multiple studies in the abstract, this phrase should not be included.

Response: We are not sure if we fully understand this comment. In this sentence, we would like to stress one of the research's innovations to use updated climate change projection.

Comment: In line 7, 11652 "(i.e. high and low flow conditions)" is really not necessary because it is explained later in the paper and most readers will understand what "extremes" means.

Response: Since the abstract should efficiently communicate the paper's essences as a standalone document, we would like to clearly explain what specific hydrological extremes are addressed in the research.

Comment: In line 10, page 11652, the authors present the annual change between +5% and +16%. This is the only numerical data presented in the abstract, and it doesn't necessarily seem indicative of the overall results of the study. Later in the article the authors seem to focus on seasonal change, but they choose to present the annual change here. This phrase should be removed from the article.

Response: We would like to retain this important piece of information in the abstract to illustrate that the overall trend is an increase in river flow under climate change at all considered mainstream station. This is a new finding (trends remain uncertain with contrasting directions in earlier studies) and we think it needs to be stressed in the abstract.

Comment: In the discussion and conclusion sections, the authors discuss the fact that certain areas show a reducing signal, and they remark that certain GCMs show considerable differences in precipitation changes and measures. While the authors devote considerable space to discussing these differences later in the article, and draw substantial conclusions from these specific results (i.e. saying that an ensemble approach is required for future hydrological assessments), they say very little about the implications of these uncertainties here.

Response: We thank the students for their useful recommendation on further discussing implications of rainfall variability and reduction at some areas in the lower Mekong. We have added more text in the discussion to sufficiently discuss the implications.

Comment#1a. Introduction: The introduction is very repetitive. It does a good job of justifying the need for study by explaining the socio-economic challenges posed by climate change. However the authors repeat their ideas in this section and present more information than is really necessary to explain the motivation for the study.

Response: Please refer to our response to your similar comment above (i.e. comment#13), where we have shortened the introduction section.

Comment#2a. Methodology: Section 2.1 is a description of the study area and the hydrologic characteristics of this watershed. This section should not be in "Methodology." It should be in its own section or perhaps a subsection under the introduction called "Study Area".

Response: Please refer to our response to your similar comment above (i.e.comment#10). We have moved the site description to a separate section.

Comment: The hydrological model described in section 2.2 calls for the maximum, minimum, and average air temperatures. However, Figure 3 shows only the projected average change in daily mean temperature. It could be interesting to see the projected minimum and maximum temperatures as well.

Response: We thank the students for their suggestion. However, because we found that projected minimum and maximum temperatures are highly consistent with the average temperature. We therefore think that providing such information would be redundant.

Comment: In section 2.3, it could be interesting to have a figure that shows the locations of the gauges used in the APHRODITE data set.

Response: We would like to restrain from providing such figure to avoid distracting readers from the paper's main objective: Changes in flow regime and hydrological extremes under climate change.

Comment: Also in section 2.3, page 11660 line 13, the authors state that 2 degrees Celsius is an unrealistic target, but in Figure 3, several of the models show predicted daily mean temperature changes of more than 3 degrees Celsius. Therefore, it seems that 2 degrees Celsius, and the RCP2.6 that they eliminated based on their assessment, should be included as a realistic scenario.

Response: We thank the students for noting the ambiguous sentence. We have revised this sentence to better motivate our selection of the Representative Concentration Pathways.

Comment#3a. Results: In section 3.1 it would be useful to show the equations used to calculate the NSE and associated biases.

Response: We have now added the equations to calculate the Nash-Sutcliffe efficiency and the discharge biases indices in the supplementary material S1.

Comment: In section 3.2, lines 18-19, the temperature patters are discussed very generally. It would be interesting to know more about the seasonal temperature changes that were observed, or to have more information about temperature changes with different scenarios.

Response: Following the reviewer's comment, we have added further information on temperature change to the manuscript. In particular, we added more information to compare temperature changes across the GCMs and RCPs, showing that a majority of scenarios project a temperature increase between 1.5°C and 2.5°C.

Comment: On page 11668, line 15, these two sentences could be combined to say "Including other bias-correction methods is out of this paper's scope because our primary interest is to understand how the Mekong's hydrology will change under climate change."

Response: We have revised the text.

Comment: Table 3. This table provides the ensemble mean, and minimum and maximum changes in annual river discharge. However, it would make more sense to present this information in the same format as Figure 6, where we see the prediction from each model, not just the min, mean, and max. Visually, Table 6 is much better at communicating the information and allowing the reader to quickly comprehend the differences between the models.

Response: We have responded to this comment, please refer to our response in the above section.

Comment#4a. Discussion: The discussion is written more like a conclusion. It is natural that following a discussion of data, the authors may draw a conclusion or two within the discussion section. However, in this section, the authors not only draw conclusions, they also discuss the implications of these conclusions. For example, on page 11667, lines 15-19, the authors assess the implications of their results on the safety of hydropower dams. This certainly does not belong in the discussion section.

Response: We would retain the discussion points on implications of the results for water management, as also suggested by Reviewer#1 (Comment#2). We think that it is highly relevant to assess and report the implications of the key findings in the discussion session, especially when our findings are of special importance for water management in practice. Indeed, providing policy and management implications is a standard, common practice seen in a majority of similar peer-reviewed publications.

Comment: This section discusses the many different GCMs that are used in the model. The authors discuss the importance of using many different GCMs, but of the GCMs used in this model, could any of them have been eliminated? Were there any that the authors felt skewed their results in an unrealistic way? For example, in Figure 8, one of the scenarios appears to be an outlier; its values are much lower than those of all the other scenarios. Would eliminating this scenario result in a more representative ensemble mean?

Response: We understand that the students wonder if any of the GCM appears to be an outlier and thus could be removed to improve the ensemble mean. Given our primary objective to quantify hydrological changes caused by climate change, we did not focus on validating the GCMs but rather select them based on model evaluations from Huang et al. (2014), Shabeh uh et al., (2015) and Sillmann et al. (2013). All selected GCMs were evaluated as performing well for the Mekong region and thus we would restrain from removing any of the model. We do agree with the reviewers that one GCM project lower values than the other models. We think it is relevant to include this information and thus added this to the manuscript. However, despite this diversion, the impact signal (i.e. dry season flows increase under climate change) remains consistent across all the GCMs and scenarios. Lastly, we think that removing one GCM would not necessarily improve the ensemble mean, because it is not clear which model has the best projection capability.

Comment: In this section the authors discuss the possible uncertainties and complications inherent in combining multiple different data sets (page 11668). This discussion clearly explains the assumptions that they made when selecting this data and the steps they took to ensure that their data set was complete. However, this discussion may be better placed in section 2.3 where the climate data is introduced. If the authors want to leave this information in the discussion, it would be wise to at least say something in section 2.3 to the effect of “limitations and potential sources of error will be discussed in section 4.” So that the reader isn’t left questioning the validity of this data throughout the rest of sections 2 and 3.

Response: Following this suggestion, we have added brief explanations and refers to more substantial discussion in Section 5 (Discussion) regarding the combination of historic climate datasets.

Comment: It could be interesting compare the results of this study to other applied simulations using the CMIP5 to know if similar results were found in other watersheds.

Response: We think that comparing impact signals with those found in other watersheds would not be very meaningful because the climatic and hydrological characteristics differ greatly across different watersheds. We decided to dedicate more space to compare our results with earlier studies for the same river basin (i.e. the Mekong). This comparison showed that (1) our results agree with and thus further solidifying the insights and (2) impact signals are more robust compared to earlier CMIP3-based assessments.

Comment#5a. Conclusion: Perhaps because of the conclusive nature of the discussion, the conclusion is very repetitive (for example, lines 10-15 on page 11666 are almost identical to lines 4-7 on page 11669). The authors need to revise the discussion and conclusion sections to better organize their ideas to fit into one section or the other.

Response: We have revised parts of the discussion and conclusion session. In particular, we have (1) shortened the summary of key findings in the discussion; (2) revised the first sentences in the conclusion section. All in all, we believed that the revision resulted in sharper and more concise discussion and conclusion sections.

Comment: The authors should take time in the conclusion to discuss what other types of data collection or modeling would be useful to continue to improve the general understanding of the Mekong watershed. After setting up a model and completing such a detailed analysis of this model and its data inputs, they have a unique ability to identify what sort of studies could be useful to continue this type of work.

Response: We thank the reviewers for their excellent suggestion. Given robust findings about increasing hydrological extremes under climate change, we have added to the conclusion our suggestion to shift focuses in both water management and hydrological researches. Regarding future studies, we added our suggestion to focus on low-probability but high-damage events, in order to better match with the information demand from management and policy domains.

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